a plant of a very different family, viz. *Solanum rostratum* (*American Naturalist*, April, 1882, p. 281): one stamen and the pistil are very long and strangely curved; four stamens are short and straight, and serve only to furnish pollen to the visiting insects; all the anthers, as I am informed by Prof. Todd, are of the same dull yellow colour. FRITZ MÜLLER

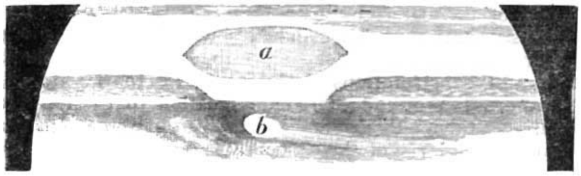
Blumenau, Santa Cattarina, Brazil, December 27, 1882

The Markings on Jupiter

AFTER heavy storms of hail on January 30, the sky cleared and the night was exceptionally fine. I observed Jupiter with my 10-inch reflector about 11h. 30m., and watched the chief markings pass the central meridian of the planet. The well-known equatorial white spot came to transit at 11h. 44m., and it was followed 5 minutes later—at 11h. 49m.—by the great red spot. These objects, therefore, must have been in conjunction on January 30, at 2h. 47m., as the greater velocity of the white spot enables it to gain 13m. 24s. on the red spot daily.

In NATURE, vol. xxv. p. 225, I stated that during the 400d. 0h. 20m. elapsed between 1880, November 19, 9h. 23m. and 1881, December 24, 9h. 43m., the white spot had completed 9 revolutions of Jupiter relatively to the red spot; the number of rotations performed by the former being 976, and by the latter 967. Since 1881, December 24, I have continued to watch the anomalous velocity of these curious markings, and find that between that date and 1883, January 30, the white spot has completed 9 further revolutions of Jupiter. From 1881, December 24, 9h. 43m., to 1883, January 30, 2h. 47m., is 401d. 17h. 4m., during which the white spot has rotated 980 times, while the number for the red spot is 971. In fact my observations since 1880, November 19, show that up to 1883, January 30, the white spot had performed 1956 rotations, as against 1938 by the red spot in the interval of 801d. 17h. 24m.

On January 30, when I last saw these markings, the red spot was remarkable on account of its great faintness. On the other hand, the equatorial white spot was extremely brilliant and conspicuous, and formed one of the most noticeable features on the planet. Observers should now keep a close watch on the red spot, as it seems likely to be on the point of disappearance, though this disappearance need not necessarily be of final character. It fortunately happens that a curious irregularity in the formation of the great southern belt will probably enable the exact position of the spot to be watched for a considerable time. This particular region of the planet, as I drew it on January 30 at midnight, was as follows:—



1883, January 30, 12h. The region south of the equator of Jupiter. *a*, The red spot; *b*, white spot.

The sketch shows that the great south belt is now double, and a very conspicuous object on Jupiter. The south half of this belt is bent abruptly to north, and runs into the other half exactly north of the preceding and following ends of the red spot. There is some explanation to this interesting feature, though it is at present involved in mystery; in any case it may possibly serve as a very accurate indication of the place of the red spot long after that object has become obliterated altogether. W. F. DENNING

Bristol, January 31

Meteor of November 17

I THINK now that more observations of the remarkable phenomenon of November 17 have been brought forward, that we cannot but candidly acknowledge that the evidence is extremely contradictory and impossible to reconcile, that is as applying to one and the same object. Altogether there is something mysterious about it. It is evident that since it appeared to reach the greatest apparent length of about 30° at York, then from all places further south it ought to have attained a length exceeding this, the more so the further south they are. The ends of the beam appeared very well defined from here, and there was very little room for estimates varying according to the observer's sensitiveness to light. If we take the observations made from Clifton, Cirencester, East Clevedon, Woodbridge, and Windsor, as they nearly all agree in estimating the length as *over* 30°, some considerably over, then these may all relate to the same object. But its appearance from York is flatly contradicted by Mr. Batson's observation from Hungerford, that from Halstead, Essex (which seems to agree with Mr. Batson's), also those from Lincoln's Inn Fields, Greenwich, and Cambridge. All these agree in contradicting the others named above, by assigning a much *smaller* angular length. Mr. Batson describes a sudden foreshortening which the meteoroid underwent when passing the moon, and since I saw it pass below the moon at practically the same time, then (on the supposition that we beheld the same object) the same shortening ought to have been visible to me; but there was not the slightest trace of any such thing. I noticed that it very gradually shortened in length (after allowing for perspective) in its journey towards the west, which is significant, and explainable if we suppose the body to have been encountering resistance to its momentum. It is impossible to reconcile all the observations, and yet most extraordinary that no single observer is known to have witnessed more than one such phenomenon at about that time except Mr. Worthington, who says he saw two *at once*. I have reason to believe that a rather similar thing was seen below the moon at about 5.30 on that night from here. I see that from Ziericksee, in Holland, a similar phenomenon was seen to transit α Pegasi (which would be at about 50° altitude, and on the magnetic meridian from there). If this was the one that I saw, then at the time that it was seen to transit α Pegasi, from Holland, it would appear to me to be just forming in the south-east, where it appeared to be about 10° above the horizon, it which case it would have to be under seventy miles high when over Belgium. But it is almost certain that it attained a height of over 150 miles during the latter part of its course. As yet (figuratively speaking) the spectra of these auroral phenomena have not thrown as much light on these things as that which enters the narrow slip of the spectroscope to print its uncertain record on the retina. I only hope that some one with a clear head and much patience will succeed in unravelling the tangled skeins of evidence which surround the mysterious meteoroid of November 17, 1882. H. DENNIS TAYLOR

Heworth Green, York, February 11

Aino Ethnology

IN an article on "Aino Ethnology" which appeared in NATURE, vol. xxvi. p. 524, and which I happened to read only a few days ago, Mr. A. H. Keane makes the following statement:—"Until the appearance of Herr Rein's large work on Japan, one of the most universally-accepted of these conclusions was that, whatever be their affinities, the Ainos must certainly be separated from the Mongolic connection. No little surprise was accordingly produced by Rein's attempt to affiliate them to the surrounding members of the yellow race. But it was soon seen that his arguments, apparently inspired by a love of paradox, were sufficiently refuted by the very illustrations of the Aino type introduced into his work."

I submit that one who has read my work upon Japan will decide, with me, that the spirit of the matter quoted is unfair, in so far as it charges me with "attempts" at affiliation, and with being "inspired" otherwise than by a love of truth—this motive being, as stated in my preface, that which induced me to write.

It is repeatedly mentioned in my book that I had never been in the island of Yezo, and those who have carefully read the whole work—including Mr. Keane, if he has done so—cannot reasonably fail to observe that I speak of the Aino tribe as one who had never visited them in their proper home, nor made them a special subject of study in any respect. My remarks upon their probable racial affinities were based upon good, and the then latest, authorities, whose names I was careful to mention. Thus, on p. 444, occurs a passage of which the following is a rendering:—"Doenitz and Hilgendorf have made thorough investigations of their (the Ainos) physical peculiarities, and have published the results thereof in the *Mittheilungen der Deutschen Gesellschaft Ostasiens*. It appeared as an undoubted fact 'that the Ainos are Mongolians, who are separated from the Japanese in a perhaps less degree than the Germans from the Romans.'"

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In the comments which I added to the views of these eminent observers, wherein I mentioned those circumstances that seemed to me to tend in the direction of their support, I was, of course, unable to include, as I would if possible have done, a statement of the opinions of such noted authorities as Dr. Steube and Herr von Siebold, the results of whose investigations have so recently been given to the press, and which are cited in the article that elicits this letter. J. J. REIN

Marburg, Germany, February 8

Hovering of Birds

I REGRET that I did not notice until to-day that Mr. Airy, in his letter published in NATURE, vol. xxvii. p. 294, specially referred to "hovering with perfectly motionless wings" as being that for which an upward slant of wind is, as he believes, absolutely requisite to enable the bird to do so.

Is the term "hovering" applicable to the examples given by Mr. Airy of gulls and hawks floating as it were with motionless wing along hillsides and cliffs?

I have always associated "hovering" with the flapping or fluttering of the wings, as is invariably noticed when terns or hawks are looking for their prey either over land or water.

February 10 J. R.

Intelligence in Animals

IN his letter in NATURE, vol. xxvii. p. 337, Mr. J. Birmingham does not mention what kind of bear it is that throws down pieces of rock "in order to catch the bareins," as told by the Kamtschadales; but the Eskimos have a somewhat similar story of the white bear, when attacking the walrus, the largest of which, with their formidable tusks, Bruin generally avoids.

The circumstance was told me by an eye-witness, a very truthful and honest Eskimo of Repulse Bay. He said: "I and two or three other Innuit were attempting to approach some walrus in winter, lying on the ice close to the water, kept open by the strong current, in Fox's Channel. As we were getting near we saw that a large white bear was before us. He had reached in the most stealthy manner a high ridge of ice, immediately above where the walrus were lying; he then seized a mass of ice[1] in his paws, reared himself on his hind legs, and threw the ice with great force on the head of a half-grown walrus, and then sprang down upon it."

The Eskimos then ran up, speared the bear, and found the walrus all but dead, thus securing both animals. I should add that the bear threw the ice as if he was "left-pawed."

Kensington, February 10 J. RAE

WHILE spending the late winter months at Paignton, in Devon, I frequently watched, through a telescope, shore birds of various kinds stalking game on the low-tide sands. These abound with sand-eels, which lie, perfectly concealed, about an inch below the surface, and are caught in the following way by the gulls.

Standing close to the water's edge, the birds tread the wet sand into soft puddles by rapid alternate movements of their feet, and when a sand-eel, thus disturbed, makes a dart for the sea, he is instantly taken by a skilful but leisurely-looking snap of the beak.

Sand-eels bury themselves without leaving any marks on wet sand, and the gulls were always seen steadily and tentatively beating over the ground in the way I have described. They took, each, a fish a minute, perhaps, and impressed me with the idea that some thoughtful ancestral gull had deserved well of his race for the invention of such an easy logical way of picking up a living. D. PIDGEON

Holmwood, Putney Hill, February 7

The Sea-Serpent

ON reading the letter of W. Steadman Aldis in NATURE (vol. xxvii. p. 338) yesterday, I was reminded by a person present that some years ago, when in Orkney, I pointed out an appearance that most people unaccustomed to witness it might have taken for a great sea-monster. This was nothing more or less than some hundreds of cormorants or "scarps" flying in a continuous line close to the water, the deception being increased by the resemblance of a head caused by several "scarps" in a cluster *heading* the column, and by the "*lumpy*" seas of a swift tideway frequently intervening and hiding for an instant part of the black lines, causing the observer to—not unnaturally—imagine that the portions so hidden had gone under water. The speed of the cormorant on the wing may be fairly estimated at thirty miles an hour or more. J. RAE

Kensington, February 10

The "Zoological Record"

I SHOULD like to point out a slight error in the last impression of NATURE (p. 311). In your notice of the *Zoological Record*, 1881, it is stated that no separate paper seems to have appeared in 1881 exclusively devoted to the group *Octactiniæ*. I should mention that Prof. Nicholson's book on "Monticulipora," his paper on the skeleton of "Tubipora," and Mr. Wilson's paper on the development of "Renilla," all appeared in 1881, and were duly recorded by me. SYDNEY J. HICKSON

Anatomical Department, Museum, Oxford, February 5

SIEVE-TUBES

A CAREFUL examination by E. Russow (*Ann. Sci. Nat.* xiv. 1882, Nos. 3 and 4) of the structure and development of sieve-tubes leads him to the following general conclusions.

In all vascular plants examined, the sieve-tubes exhibit a remarkable agreement in structure, always expressed by the presence of callus. The sieve-punctation appears to be wanting in *Isoetes*, and possibly also in the Marattiaceæ. It is not, when present, confined to the sieve-tubes, but occurs also in the parenchyma of the secondary liber. It is often difficult to decide whether these punctations are actually perforated; but this is clearly the case wherever the sieve is traversed by callose cushions or striæ, or by connecting filaments; the presence of callus is not of itself sufficient to indicate perforation, for its formation certainly precedes the perforation of the membrane. In conifers the punctations between the sieve-tubes and the cells of the medullary rays are provided with callose cushions only on the side of the sieve-tubes, and the punctations remain closed.

The development, accumulation, and final disappearance of the callus indicate that it is not a product of transformation of the cellulose, but that it is separated from the contents of the sieve-tubes; its accumulation round the perforations is proportionate to the freedom and duration of the intercommunication that takes place through them; this communication probably continues as long as the striæ of the callus remain clearly developed, and ceases when these disappear, close up the sieve-pores, and end the function of the sieve-tubes.

In gymnosperms and vascular cryptogams, mucilaginous filaments are never to be seen traversing the callose cushions, although there is always a certain amount of communication between the elements of the sieve-tubes. The special function of the sieve-tubes is probably always maintained wherever striæ cross the callose cushions. The large number of plants in which the sieves are traversed, both in summer and winter, by mucilaginous filaments, and the large number in which no such filaments are at any time observable, contradicts the idea that the function of the callus is to close the sieve-pores during the dormant season.

Much less callus is deposited in the sieve-tubes of closed fibrovascular bundles, especially in permanent organs, than in those of open bundles which increase in thickness from the activity of their cambium. This difference corresponds to a difference in the nature of the contents, and in the duration of the activity of the sieve-tubes. While in gymnosperms and dicotyledons the active period of the sieve-tubes rarely exceeds two years, in monocotyledons and vascular cryptogams it lasts as long as the organ itself. A stem of *Alsophila*, at least

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[1] It may be questioned how the bear could find a lump of detached ice. The strong current mentioned is constantly breaking up the ice into small pieces.

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